An accurate observer related to me the case of a lady whose iris changed colour in bright sunlight.

These few instances seem to show that the behaviour and properties of pigment-cells are independent of the protective functions for which they have, in some cases, been specialised and augmented by the action of natural selection.

It seems a pity that Mr. Murphy should write on a question in natural history without making himself better acquainted with what is known on the subject. FRANCIS DARWIN

Down, Beckenham, Aug. 14

IN the last number of NATURE Mr. J. J. Murphy states the difficulty which he finds in accounting for the rise of intermittent variations upon the theory of natural selection. He can understand the origin of a white species from a brown one or *vice versâ*, but not of a species which, like the ermine, is at one season brown and at another season white. He speaks of "facts of colour which it seems impossible for natural selection to produce, from the infinite improbability of a first variation ever occurring." From this mode of expression one might fancy that Mr. Murphy had for the moment forgotten that natural selection is in no way concerned with producing, but acts only by preserving, variations. As in a great number of instances we are ignorant of the precise antecedents which produce variation, whether chronic or recurrent, in such instances, we must be left at liberty, if we choose, to invoke the special action of "a guiding intelligence." The case, however, of an animal which changes its colour with the season does not seem to be one of very exceptional difficulty. It is only necessary to suppose that the animal became possessed of pigments liable to be acted on in the required direction by the seasonal changes of light and heat. It might well be that with some animals the influence of the same changes would be in a direction just the opposite of what was useful to them. In that case the variety would stand but little chance of being preserved. Similar explanations hold with regard to the vegetable kingdom. I have now before me drawings of *Sempervivum spinosum*. The summer rosette is bright green in colour, with the leaves expanded, while the winter rosette is a compact little ball of a dull purple. Thus the plant prepares itself against the cold of winter and the dearth of nourishment which that season brings, but it is likely enough that cold and dearth in the first instance led to the variation in the plant from its summer habit.

In human beings the hair is said sometimes to turn white from sudden grief or terror. Liability to such a change does not probably carry any such advantage to the human species as would make it likely to spread and develop itself further. But in the little shrimp commonly known as *Mysis chamæleon*, we can at least conjecture that a very solid advantage might follow from a similar characteristic. I have sometimes bottled live specimens of this little creature while it was of a dark purple colour, and presently after lost sight of it, the fact proving to be upon closer inspection that it had become almost completely transparent. Among its ordinary enemies the loss of colour might often save its life, in which case natural selection would tend to preserve the aptitude, although the aptitude itself, like the bleaching of human hair from grief, has no connection at the outset with the advantage of the species.

Torquay, Aug. 14 THOMAS R. R. STEBBING

MR. MURPHY'S letter (NATURE, vol. xiv. p. 309) opens up a wide field for speculation. The class of cases to which he directs attention constitutes what I have designated "*variable* protective colouring," and in a paper communicated to the Zoological Society (Proc. Zoo. Soc., 1872), I attempted to show that such cases came to a certain extent within the scope of natural selection. The line of argument pursued is briefly as follows:—Natural selection, working solely for the good of a species takes advantage of all beneficial variations, no matter how they may originate. In but very few cases can the cause of any particular variation be assigned. Natural selection works only on the variations presented to it, the causes of such variations appearing to us, in the absence of observational or experimental evidence, mysterious. If, then, a species deriving advantage from protective colouring under one set of conditions, finds that the conditions vary periodically or irregularly, thus rendering that mode of colouring useless or even disadvantageous, it clearly becomes advantageous to the species to possess a *power of adaptation*. By this means only can *varying* external conditions be met, and it is upon this *adaptive power* that I venture to think the action of natural selection has in these cases been exerted. That the particular cause of such variation cannot be assigned, no more weakens the natural selection argument in these cases than in ordinary instances of permanent protective colouring, the possibility of which having been brought about by the "survival of the fittest," Mr. Murphy seems disposed to admit.

One argument in favour of the natural selection theory of protective colouring appears, so far as I am aware, to have been overlooked. It has been urged that granting the power of natural selection to produce a general resemblance in colour, &c., to inanimate objects, it is difficult to see how the highly perfect finishing touches (instances of which are familiar to all naturalists) could have been imparted by this same agency. To this it may be replied that the marvellously perfect resemblances which we witness have not been brought about to deceive *our* visual sense, but that of far keener-sighted foes whose very means of subsistence may depend upon acuteness of vision.

Apropos of Mr. Power's letter in the same number of NATURE, I have recently had an opportunity of observing how closely the larva of *Trachea piniperda* resembles in the longitudinal green stripes the needle-shaped leaves of the pine on which it feeds. I observed also an equally good adaptation in a larva of *Agriopis aprilina*, which when resting on a lichen-covered oak trunk was barely discernible from the lichen on which it rested.

Belle Vue, Twickenham, Aug. 12 R. MELDOLA

Antedated Books

THE grievance pointed out by your correspondent "F.Z.S." is a real one. Nevertheless I trust that the writer is himself free from the charge that he so glibly brings against a brother naturalist of endeavouring to obtain for his generic titles an "unjust priority of fifteen months over what they are entitled to." I am sorry that there should be a Fellow of the Zoological Society who believes me capable of doing this, but, as the charge has been thus publicly made, I lose no time in flinging it back upon my anonymous accuser. The new edition of Layard's "Birds of South Africa" was announced to appear in six parts, and the first was published in May, 1875. The number of wrappers required for the six parts was printed off at the time, and "F.Z.S." will find that Part 2, which was published last autumn has precisely the same wrapper as Part 1, and this is the case with the part now issued. I admit that it would have been better to have altered the date on each wrapper in writing; but this, probably, did not occur to my publisher, who is doubtless not aware of the importance attached to the law of priority by "F.Z.S.," your correspondent, who, apparently, in his hurry to attribute an unworthy motive, has scarcely taken the trouble to look beyond the cover of the book. Had he done so he might have been satisfied that the letterpress contained abundant evidence of having been written long after the date which he would have the scientific world believe I had endeavoured to claim for its publication. Such an attempt would be absurd when documents are quoted in the letterpress which were *not in existence in the year* 1875.

May I at the same time reply to a paragraph of your reviewer (p. 318) on the "Birds of Kerguelen Island." This pamphlet deserves all the praise which the reviewer bestows on it, but in his endeavour to disparage his own countrymen, and to trumpet the superior energy of American ornithologists, he seems to have done an injustice to Mr. Eaton and myself. Two new species were mentioned by Dr. Coues, viz., *Æstrelata kidderi*, which Mr. Salvin (*Orn. Misc.*, p. 235) shows to be *Æ. brevirostris* (Less.), and secondly, *Querquedula eatoni* (Sharpe). This latter name looks as if the English ornithologists had not been so far behind their American brethren, after all, if the description of the new Teal was available for quotation in Dr. Coues' work! R. BOWDLER SHARPE

British Museum

FULLY agreeing with "F.Z.S." in reprobating the evil practice of which he complains, I think that in the particular instance he cites, of the recently published third part of the new edition of "The Birds of South Africa," he will, on looking again at its wrapper, see that the information it affords is so contradictory as to be worth nothing. The first words upon it are "To be completed in *Six Parts*;" but on its second page we read that the publisher "has decided upon issuing this work in *four* parts!" Which of these statements is to be believed? In justice to the publisher, however, it is to be observed that the number "3" is not printed, but inserted with the pen, in the

copy I have received, and also that the "May, 1875," has a line drawn through it. While on the subject let me add that the Zoological Society itself, in its "Transactions," sets a bad example in this respect. Each paper bears a date at the foot of its first page, but the date is likely to be misleading in years to come, for it is that of the *printing* off the sheet—an essentially private matter, with which the public has nothing to do—and not that of the *publication*. Another F. Z. S.

Meteor Observations

A BRIEF summary of the August (Perseid) meteor observations at York may be of interest.

Watch was kept on the 10th, 11th, 12th, and 14th. The night of the 11th was very hazy, the nights before the 10th cloudy. There was also much moonlight, except on the 14th. Yet, after making all due allowances, Prof. Herschel thinks that this year's shower indicates a minimum; the last decided minimum being in 1862.

The hourly number on the four nights mentioned were, for one observer, 22, 8, 12, 15, respectively. Perseid radiant and sub-radiants gave 18, 6, 9, 7. Thus, as the shower progressed, there was a regular decrease in the number of Perseids. The apparent exception of the 10th was due to the haze. Prof. Herschel gives 15-20 as the hourly number in Kent. On the 14th half the Perseids came from Mr. Greg's sub-radiant at γ Cassiopeiæ.

In the south large meteors appear to have been scarce. Here eight, brighter than 1st mag. stars, were seen. One, a bolide, low down in the N.W. was very fine. A meteor in the south-west, brighter than Jupiter, was observed by Mr. Waller at Birmingham as a very brilliant object.

The total number observed at York was 105, and 90 of these were mapped. Of the latter 66 were Perseids, 43 with trains. On the 10th five other radiants produced eight meteors out of 53; viz., Cygnus, three; Pegasus, two; Polaris, one; Draco (Hercules), one; and Ursa Major, one. Fifteen meteors on this night were as bright, or brighter, than a 1 mag. star. Only two of 4th mag. brightness were seen, in consequence of the moonlight.

Of meteors stationary, or nearly so, three were mapped:—A Perseid on the 12th at $\mathcal{R}$ 32½ and δ + 58⅔, its train lasting 2½ secs.; on the 14th a Cygneid at $\mathcal{R}$ 306° δ +35°, and an unknown radiant, probably near 5 Vulpeculæ, gave the third at $\mathcal{R}$ 295° δ + 28.

Three meteors unmistakably confirm Mr. Greg's sub-Perseid radiant by γ Cassiopeiæ, whilst several others probably radiate from the same. The radiant, Greg 83, by η Draconis, gave two meteors on the 12th and one on the 14th. It is put down, however, as lasting only from July 12–31.

Six Perseids on the 10th, and four on other nights, seem pretty clearly to indicate a sub-radiant at $\mathcal{R}$ 50°, δ + 40°, near α Persei. The rest, as Prof. Herschel also noticed, shot very constantly from the chief radiant, between η and χ Persei. Here, however, η Persei seemed the most central point.

York, Aug. 15 J. EDMUND CLARK

THE FRENCH ASSOCIATION

IN addition to the notes already given with regard to the forthcoming meeting of the French Association at Clermont, the following particulars relating to the Puy-de-Dôme (furnished by our correspondent there) will doubtless be found interesting:—

Clermont, August 13

The Puy-de-Dôme is connected with most important scientific events, which render it notable amongst more lofty mountains.

Pascal, in 1644, then quite a young man, was apprised by Père Mersenne, the celebrated friend of Descartes, that Torricelli had invented his tube. The then admitted explanation was that nature abhorred a vacuum.

He entered into a correspondence on the subject with Father Noel, a Jesuit professor of natural philosophy in the College of Clermont. Father Noel contended against the very existence of the vacuum, and asserted that the so-called vacuum was filled by luminous matter entering through the glass. Pascal answered by arguments worthy of his genius, and to be recommended for consideration in the discussion about radiometers. He said, "As the nature of light is known to neither you nor me, and as it is very likely it will always be so, I see it will be long before your reasoning acquires the force which is necessary to its becoming the source of any conviction." After having uttered this opinion he reflected more fully upon the subject, and was led to believe that the surplus height of mercury in the tube was equivalent to the weight of the air which could not reach the molecules, being intercepted by the resistance of the glass. This led him to inquire if air-pressure was not lessened by taking the Torricellian tube to the top of a mountain. The experiment was made in Paris first on the top of St. Jacques la Boucherie Tower and Notre Dame. As the difference was found to be only a few lines, Pascal sent his brother-in-law, Perrier, who was a counsellor in the Cour-des-Aules at Clermont, to the top of Puy-de-Dôme with a Torricellian tube. Clermont was supposed to be at an altitude greater than Paris by 400 toises; Font-de-l'Arbre is a village in the vicinity of the mountains where carriages are obliged to stop, at 250 toises from Clermont, and 250 toises from the top of the mountain. All these measurements are incorrect; a toise being 1·94 metres, we find the following differences:—Paris, 60 metres, Clermont, 407; difference, 347 metres, instead of 776, as assumed by Pascal; Puy-de-Dôme, 1,465. Difference between Puy and Clermont = 1,058 metres; according to Pascal only 952 metres.

The loss of mercury from Couvent des Minimes to the top of Puy was found to be 37½ lines; at Font-de-l'Arbre a diminution of 14½ lines from Minimes. A line is equal to 2¼ mm.

Perrier discovered no difference, owing to the wind or state of the atmosphere. Such was not the opinion of Pascal, who discovered that the mercury varies according to the atmospheric conditions of the air. But Perrier was only an amateur experimentalist, and his special ideas had little weight with his clever brother-in-law.

In order to ascertain the fact, continuous observations were made at Clermont, by Perrier, during the years 1649, 1650, and 1651. They were simultaneously made at Paris and at Stockholm, where Descartes was then living at the court of the famous Queen of Sweden. They were continued by Descartes up to the time of his demise.

It is strange that the Pascal experiments were made the very year when Torricelli died, and the results published only in 1664, two years after Pascal's death.

THE SCIENCE DEGREES OF THE UNIVERSITY OF LONDON

WE have received from the Registrar of the University of London a copy of the Report of a Committee, and the new regulations which have been introduced in harmony with that Report, in the examinations for the science degrees. From a perusal of the Report, which we subjoin, all will feel how much is gained by the prompt action of the Senate of the University in so speedily modifying the plan of their examinations in accordance with the experience which they have obtained during the last seventeen years. It is not, however, only experience in the examination of science students which has led to the necessity for change, but the stimulus which has been given to the teaching of physics and biology, by the founding of science degrees and otherwise, has so altered the method of teaching these subjects that what was expected to be known formerly is quite different from that taught by the most able exponents of the subjects at the present time.

No change has been made in the Examination for the Doctor of Science degree, which we regret, because in the Report of the Duke of Devonshire's Committee on Scientific Education great stress was laid on the importance of obtaining an original thesis from each candidate.

The Report of the Committee runs as follows:—